

April 18, 1953

Dear Phil:

Yours of the 15th just received. You have full and sympathetic understanding concerning distractions from lab. work. There is no great pressure for reports on most of the things that have been sent.

I would like very much to have the abortus-equi and their a phases that you mentioned. I imagine, from the results obtained so far (e.g. SW985, abortus-equi --x SW666, a:--) that they will behave fairly typically, i.e., the a as a first phase homologue; the enx as a second. Para-A is likely not to go too well. Even the XII₂ strain does not adsorb PLT22 ~~veryw~~ very well, but I am only just about to try some transductions to it. I'll be happy enough to send Kauffmann the phage, but what in the world does he want to do with it?

When I wrote N97 --x B. miami, I meant the b phase; sorry not to be more explicit. The experiment has just been done, and the result explodes my theory: b:1,5. Something along the lines of your remarks may well be the answer. The difficulty is putting them in the framework of a more detailed genetic theory. The b:i culture does not, however, exhibit normal phase variation. The i goes to b readily enough, but not the reverse. This is like the 1,2 phase from my "N97", which also goes ~~to~~ b (or rather to a peculiar b+g33, the latter reaction much stronger than the original b phase) which is then quite stable. There are some more tests to make on the homologies of the "phases" of the b:i, but you can pretty well discard my hypothesis that the java is $H_1^{1,2} H_2^b$ (vs. $H_1^b H_2^{1,2}$ for typical paratyphi B).

The SW986 peculiarity may be due to the TM strain. I have some derivative cultures which behave as if they go i: 1, 1,2 : 1,2, and SW 986 came from Abortus-equi --x one of these. I am wondering whether a microscopic motility test couldn't establish the possibility of mixed-phase cells.

As I am suspicious of the identity of my "N97" it would be advantageous to start with a fresh one of these.

Hastily,

Joshua Lederberg